International Studies Review (2004) 6, 511–526

THE FORUM

Courage Versus Caution: A Dialogue on Entering and Prospering in IR

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Introduction

AYDINLI: As a newcomer with a recent PhD trying to make my way in the community of International Relations (IR) scholars, I sometimes feel like a juggler with too many balls in the air. How can I possibly keep aloft the many different roles I see for myself in this community? I feel I must be a good teacher and guide students in gaining disciplinary knowledge, yet I believe I should also be a thoughtful and well-informed intellectual who can possibly positively influence political developments in my local Turkish environment as well as internationally. Moreover, it is obvious that I must be a researcher, produce quality scholarship, and hope in doing so that I contribute to the accumulation and progression of disciplinary knowledge. Finding an appropriate balance between these roles, or deciding whether one or more should take precedence, presents me, and no doubt other young scholars, with very difficult choices.

Furthermore, within each of these roles that I see for myself, I am faced with additional choices concerning not only the messages I present, but also the ways in which I want to get these messages across. If I consider only the role of researcher, I need to ask whether I want to try to forge my way alone or whether I would be better served by clearly identifying myself as part of a particular research community. And if I choose the latter, then which research community should it be? Should I pay homage to the most traditional ideas so that I remain a part of the core of the IR community? How do I act so as to retain my own perspectives while at the same time becoming an accepted member of this community and survive? In other words, how do I build my professional identity?

We emergent scholars seem to be picking our way through if not treacherous, then at least problem-ridden territory on our journey to building up our professional identities. We are faced with choices between various theoretical identities—from the broadest epistemological and paradigmatic questions (for example, should we take the positivist or postpositivist route?) to more discipline specific positions (are we a realist? a neoliberal? or perhaps a constructivist?). There are also

Published by Blackwell Publishing, 350 Main Street, Malden, MA 02148, USA, and 9600 Garsington Road, Oxford OX4 2DQ, UK.

the practical elements of one's identity. In other words, should we locate ourselves firmly in the academic world of the university, in think tanks, or in policy centers? Should we choose to emphasize our teaching or our research? There arises a whole set of different choices if we happen to be a foreigner trained in the West, for then we must also decide, among other things, whether to try and locate ourselves professionally in our native "local" IR circles or in the greater international IR community.

It is hardly unreasonable for an emergent scholar—to the extent he or she is conscious of these choices and the stakes they involve—to find this socialization journey into the discipline a daunting one. The journey takes on added complexities when we begin to consider other questions such as how much we are in control of making these choices and how much they are determined for us by restrictions that range from the more concrete (for example, language abilities, institutional requirements, access to technology) to the more abstract (for example, the limits of one's disciplinary training, one's personal tendencies when considering group membership, one's intellectual inclinations).

For myself, I felt it would be an interesting exercise to seek out the ideas and guidance of someone who had not only made this journey, but who had done so in what might be called an adventurous manner and in the process given evidence of a willingness to reflect upon his journey. From both his published literature and from personal contact with him, I have an impression of James N. Rosenau as a scholar without rigid fixations on certain concepts or ideas and as a scholar who has been open about his professional journey. It was when I read the final chapter of his book *Distant Proximities* (2003), in which he presented a critical self-reflection of his own academic journey, that I became fully convinced that he would be an ideal person from whom to seek insights about various issues and dilemmas puzzling and troubling me in my professional life.

In that chapter, "A Transformed Observer in a Transforming World: Confessions of a Pre-post-modernist," I was deeply struck by Rosenau's forthright discussion of personal and intellectual traits that he believes have shaped his professional life, including his apparent major shift from doing research in a "scientific" tradition to one he calls "relaxed-science." In explaining his own professional development, he notes his openness—or perhaps need—to seek new and different approaches, his methodological conviction as to the inseparability of individuals from the larger contexts in which they participate, and finally his observations over the latter part of the twentieth century that states have been losing control and therefore his deepening commitment to focusing his scholarly inquiry on the "frontier" between domestic and foreign affairs. I found myself also intrigued by the case he builds at the end of this chapter for methodological and theoretical bridge-building, which he bases on his belief that we can and must seek consensual understandings if we are to accumulate knowledge.

The following dialogue thus began in a sense as I read Rosenau's confessional chapter and filled the margins with questions and comments, which I subsequently communicated to him. We went on to exchange ideas on various aspects of the processes of newcomer socialization and professional identity formation. Very much in relation to these processes, we spoke as well on issues of change (for example, how open can a scholar be to change in his/her identity, research agenda, or perspectives? What are the costs of such changes?) and methodology (for example, how and why do some scholars attempt so-called "jail breaks" from firmly entrenched methodological identities?). The resulting dialogue, presented here, is therefore structured broadly along these themes of socialization, methodology, and change. Given that this dialogue includes various references to passages or ideas in the "confessions" chapter of Professor Rosenau's *Distant Proximities* (2003), the interested reader may find benefit in reading his book as well.

On Becoming an IR Scholar

AYDINLI: The epigraph to your autobiographical chapter in *Distant Proximities* (2003) says that "the forward-moving are doomed to be misunderstood." As such, it distinguishes only between so-called "forward-moving" individuals and presumably less-progressive types. I wonder whether the truth of the epigraph might be more or less significant for scholars at different stages of their professional development. As a newcomer myself, I have to ask: Can a newcomer in the discipline afford to be unconventional? Can he or she wander innovatively through the "terra incognita," or in doing so will he or she run the risk of being ignored? In point of fact, is the discipline more accepting of the "new" when it comes from the experienced scholar?

ROSENAU: These are good questions. I am far from sure of how to respond. There are, as you imply, orthodoxies that pervade the fields of IR and political science (and perhaps all the social sciences). They consist of standard hurdles that newcomers have to jump over in order to progress further down the track. The hurdles are most conspicuous when it comes to writing an acceptable dissertation, but they can also block the route to tenure by requiring publication in established journals, reference letters written by established scholars located in established universities, and so on. Thus it can be difficult for any newcomer to get accepted at the outset. Now and then (but not often) an article or book might be so compelling that the author's fledgling status is ignored and he or she moves ahead quickly. Graham Allison's *Essence of Decision* (1971) is a good example in this regard. To repeat, however, few newcomers make a mark with their first publications.

It follows that the "new" is more likely to gain acceptance when it is authored by more experienced scholars. But for more experienced scholars to break out of their long-existing mold is extremely difficult. They develop a deep stake in what they have done for which they have won praise and approval. As a result, they tend to be unable and unwilling to dare, to try new approaches, and to probe unexplored questions.

AYDINLI: This seems to spell a dilemma for the discipline in that those most poised to make changes (the newcomers) are in the worst position to do so because they lack widespread legitimacy or name-recognition. Those in the best position to make changes and have these arrest attention are the least likely to do so. The question, of course, comes to mind: how did you manage to make the changes you note in your chapter when you were at the time an experienced scholar?

ROSENAU: Exactly how I managed to move beyond my work in comparative foreign policy and the rigorous (today I am inclined to use the word "rigid") approach to scientific inquiry that I was espousing at the time is not clear to me. It is certainly not the case that I always planned to move in new directions once I got tenure. Indeed, it was not until more than two decades after I got tenure that I began to move in new directions with the writing of *Turbulence in World Politics* (1990). In the last, autobiographical chapter of *Distant Proximities* (2003), I offer an explanation for the move, but it is far from a thorough explanation. The main reason set forth in that chapter is that I had become restless over the discrepancy between the way world politics seemed to be evolving and what I understood to be its foundations. A number of events seemed so contrary to my training in and grasp of the field that I started an "anomaly" file in which I put clippings about events for which I had no ready explanation. The anomaly file grew rapidly as the 1980s wore on and I decided it was time to go back to the drawing board and develop a new theoretical perspective that was not negated by the anomalies.

But this set of statements is hardly a full explanation. I can think of numerous reasons not mentioned in that chapter. A major one concerned a sense that I did not have the methodological skills—or was it fortitude?—to empirically implement the theoretical framework I had developed in the 1960s for comparing the foreign

policies of different countries (Rosenau 1966). Several colleagues around the country were implementing the framework through the compilation of event data sets that, in turn, allowed for sophisticated quantitative analyses. In a sense, therefore, I felt free to follow the dictates of the anomaly file. In short, I experienced considerable intellectual growth.

Aydinli: Would you say that there were any costs to this change to a non-mainstream position?

Rosenau: Yes, I suppose you can say there were "costs" that attached to having broken with the orthodoxy and gone off in unexplored directions. I see myself as increasingly marginalized in the US political science and IR communities, as a bit too far out to be sought for conferences, speeches, papers, and so on. I sense that my work is now appreciated more by people elsewhere in the world than in the United States. It would seem that colleagues abroad are less bound by our US orthodoxies and thus more ready to find merit in my explorations. The same might also be said about the work of Richard Ashley at Arizona State. I have the impression that his work is more often pondered abroad than here at home.

Aydinli: As someone from elsewhere in the world, I might suggest that in the mainstream of North America, IR still tends to be exclusive of "other worlds" (the periphery as well as peripheral images of world politics). Because you do not represent that mainstream, you also do not fall into the trap of US-centrism. For this reason, among others, you are appreciated abroad where people seem to try and look at things in a more global perspective, or at least not in a US-centric one.

Going back to the first part of your comment, I wonder what you think are the primary reasons for your marginalization. Might it not come in part from the fact that your work in the 1990s does not seem to fall into a single, prominent, and clearly identifiable theoretical path? As students of IR, when we are introduced to IR theory we are presented generally with a categorized list that, with some variations, basically goes from realism to liberalism to Marxism, constructivism, and critical theory. We also see names of certain scholars clearly identified with these primary categories, the obvious ones being people like Waltz, Keohane, Wendt, and so on. But I wonder where you fit in these categories? Because you do not seem to clearly fall into any single one, it is perhaps to be expected that you would be less likely to be familiar to students. I also suspect that this instructional method of introducing these set categories and certain individuals within them provides a strong push for students to position themselves explicitly within a single theoretical category.

When I say that you do not seem to fit into any single category, I seem to be going along with your concluding self-assessment in the confessions chapter of being a bridge-builder, a position you say was reached after passing through an earlier position of single-minded "feistiness" (which we could equate with the newcomer's desire or feeling of being pressured to fit into a single category). You say that you are not sure whether it is possible for someone to skip the earlier stage. Which raises the question, is such a transformation purely a matter of socialization (you have to live it to experience it) or can mentoring ease or speed it along—essentially, can we guide students in a way that they can skip over the "single-minded" period and the need to find a niche? Should we? It seems like the beginning of an answer may lie in asking what the purpose of the "single-minded" period is; arguably, it is the emergent scholar's attempt to carve out a niche for him/herself. Perhaps the problem does stem in part from our training, which seems to teach that finding a niche (or even just discovering a gap in a literature review) is a process with a confrontational logic, involving the discrediting of others and the putting forth of something new and different.

Rosenau: Once again you ask penetrating questions, though in some ways they are the same questions with the added dimension of whether a break with orthodoxy can be taught, whether it is possible to encourage and mentor one's students

to be independent and venturesome. It is an excruciating question because, on the one hand, encouraging one's students to conform to the established orthodoxy is an anathema for me, but, on the other hand, one is reluctant to have them risk their careers because they listened to you and got set back because they broke with the prevailing orthodoxy prematurely. I had one very bright graduate student who came to the conclusion on his own that he could not and would not write the standard dissertation, with the result that he left graduate school. I regretted his decision because he was very gifted, but I also understood and did not try to talk him out of it. Conceivably, he might have stayed on, swallowed his pride (or, as a favorite colleague used to say, "risen above his principles"), and written a dissertation that minimally met the orthodox standards. I suppose that is always an alternative, but it is risky because one is never quite sure what the minimal standards are or whether they will still be in place when it is time to defend the dissertation.

AYDINLI: Perhaps the idea of bridging, synthesizing, and recognizing value in old ideas only comes as scholars themselves grow older!

ROSENAU: Does aging have anything to do with moving on? Probably so, though I do not recommend feeling constrained until one reaches their fifties or sixties. The process of moving on occurs subtly and slowly, perhaps facilitated by some positive feedback one gets after the initial move, as I did as a consequence of publishing my *Turbulence* book in 1990 when I was sixty-six years old. That work caused a stir because it offered a new systematic approach to grasping the underlying dynamics of world politics and seemed so relevant to the changes that followed the end of the Cold War. For a while it led to invitations to write papers that applied the turbulence model to particular situations. Encouraged by the favorable response to this book and building on the papers I subsequently wrote that extended the model, I was then led to writing *Along the Domestic-Foreign Frontier*, which came out in 1997. So, looking back, in my case it does seem that I became more innovative as I aged, though let me repeat that I do not recommend waiting until later in life before daring to depart from established modes of inquiry. If one has, as I like to put it, "a fire in one's belly" that can only be stoked by efforts to push back the frontiers of knowledge, there is every reason to rekindle or add fuel to it as one gains a sense of intellectual identity and security (and tenure!) in his or her thirties.

AYDINLI: Going back to the idea of developing a clear, single theoretical identity, I find it interesting to consider how one goes about doing this; obviously the act of citing others in our works is a clear way of identifying where we stand. As newcomers, it is like registering ourselves in a new neighborhood—citations acting as the code for the language among the members of that community. They not only tell others where you belong, but they may have practical implications as well because, for example, referees of journals, conferences, and grant submissions might look at them. We could assume that if a referee falls into the same community, your submission is more likely to get accepted. Or from the opposite side, I have seen in my recent experience as co-editor of a journal that some authors request their manuscripts not be sent to particular scholars—presumably those from other "communities."

ROSENAU: I did go through a confrontational stage with paragraphs and articles criticizing the proponents of the approaches and concepts that I found wanting. However, today I no longer need to confront other schools of thought and their devotees. In IR, for example, it has become standard to cite Kenneth Waltz either favorably or critically. I no longer cite him at all. Neither he nor Robert Keohane is in the list of authors in the index of the *Distant Proximities* (2003) book. I may be wrong in responding in this way, but my new perspectives have simply not yielded to a need to build on or reject earlier works. Of course, whenever earlier works contain findings or concepts that serve to enlarge what I am trying to say, I do cite and expand on them. Such citations, however, range widely beyond the IR

mainstream as my grasp of the field has expanded to include work being done in the social sciences other than IR and political science.

Aydinli: These last comments seem to support the idea that you are not a part of a single theoretical brotherhood. As a result, you do not need to "take an oath of membership" via citing certain works or ideas or trying to discredit others. Which brings me back to the bridge-builder identity. Why is it so hard to have a bridge-builder identity? Why do scholars seem to cling to a particular theoretical/methodological disciplinary identity and how can we free ourselves from these? Even those who oppose established dichotomies in IR seem themselves to have just created an alternative identity to emphatically espouse. It seems like there is a great deal of talk about building, consensus, compromise, yet very little action in that direction.

Examining my own experience, I always felt that the questions/issues I observed in real-life experiences led me to the inquiries I pursued and that the nature of these inquiries determined the ways in which I would conduct them. Now I am feeling increasingly pushed into defending certain positions that I am not even sure I fully espouse, simply because they seem to be under an excessive reactionary attack. For example, consider certain broad theoretical positions. In my current work context, I find myself defending positivism against increasing numbers of colleagues who rather fervently expound postmodernist or postpositivist approaches.

Somewhat similar to this general theory-related dilemma is another that I find myself confronting particularly as an IR scholar from the periphery, namely, am I to be an importer of theories or should I strive to be a creator of theories out of my indigenous context? Predominantly I see colleagues taking the first route—an issue that has received some discussion in the literature, including in an article I coauthored (Aydinli and Mathews 2000). These colleagues basically adopt the latest or most fashionable theoretical trend and set up a local franchise. It seems to be a pragmatic choice—the ingredients and menu are set, so to speak, as are the customers. Choosing the indigenous theory-creation route seems the more challenging—both intellectually and practically. It rules out the ease of having an obvious starting literature from which to proceed methodologically or of having obvious journals to which to send your works, and therefore seems to render you less competitive against the importers. Perhaps the most distressing aspect of this problem, both personally and from the perspective of knowledge accumulation in the discipline, is that I know there are scholars throughout the periphery who are choosing not to join the franchisers. I have met people like this from India, Russia, China, and Finland, just to name a few. But they, we, are too dispersed, or perhaps simply too unaware of each other's existence, to recognize the possibility of coming together in a collectivity that might have more influence. For now, the importing route remains the dominant one, and, at least in the Turkish case, this trend seems unlikely to change in the short run. There are the practical reasons for importing theories, but there are also socially related issues for doing so in the periphery, such as the dominance of the elite in the local IR discipline and their familiarity—and subsequent attraction—with things foreign.

In considering these various dilemmas and how ultimately I will choose my way among them, I, of course, wonder whether you have ever experienced unease with the routes you chose or the positions you chose to assume, and have you ever felt any pressures to go a certain way?

Rosenau: Since I turned away from orthodoxy I have not once experienced unease about the turn. The pressures are all now within me. The fire in my belly burns too furiously to be anxious about the departure from orthodoxy. Indeed, it is that very fire that allows me to resist pressures to, as you put it, "go a certain way."

Aydinli: Let me interrupt for a second. I wonder if this attributing of all the pressures to coming from within is because of your position as a senior scholar.

It seems that for the newcomer the pressures come from outside. The more senior scholar may have a greater freedom to no longer care about the outside.

ROSENAU: As I have already indicated, you are correct: being older and more secure provides a freedom to go one's own way, or at least a freedom not to adhere to established ways. But there is more to it than that. This process also involves the development of some basic intellectual convictions that evolve with time and experience. In my case these convictions, this fire in my belly, add up to a desire, perhaps even a compulsion, to demonstrate that it is possible to clarify the human condition through social scientific analysis. Social scientists share this conviction, but a broad swath of publics everywhere is either not familiar with or antagonistic to the notion that people can be studied systematically. My social science is not conventional, but it is social science and not merely naval gazing. I see myself as a theorist who values empirical materials and their validation. For me, theory involves generalization about one or another process or institution that sustains one or another aspect of world politics; by empirical materials I mean what I call "observables," those data that are descriptive of how the processes or institutions operate.

With regard to building bridges, let me say that it is hard to do and rarely happens because most scholars are so fully rooted in their paradigms that, in effect, they are incarcerated in conceptual jails and have no interest in finding escape routes (see Chapter 2 in my *Turbulence* [(1990)] book for an elaboration of this point; it is called "Justifying Jailbreaks"). A good example is evident in my occasional involvement in efforts to collaborate with economists. Most of them seem to have no or little interest in collaboration because (a) they have their own paradigm that lacks a need for political variables and (b) they cannot advance (or at least they think they are unable to) in their fields by going outside and working and publishing with colleagues in the other social sciences.

But despite these problems I have managed to build a few bridges. In addition to forming close working relationships with sociologists and psychologists, early in my career I edited a large book of readings (some fifty reprinted articles and a few original pieces) that served as a bridge by drawing widely on the social sciences and that for years enjoyed considerable success (Rosenau 1961). It can fairly be said, I think, that a whole generation of IR graduate students was assigned this reader in their first year of graduate study and I still encounter people high in government and academe who say, in effect, "I am glad to meet you and put a face to a memory. I read (and suffered with) your reader way back in graduate school." Unfortunately three decades later the publisher let the book go out of print and thus most of those in today's generation do not know of its existence. More recently, I was among the founders of the Globalization Studies Network, which is, in effect, a consortium of globalization centers and which encompasses some eighty-five centers from a wide variety of countries in all parts of the world. The Network is an excellent example of a bridge!

In sum, it follows that while you may still be a newcomer, I urge you to resist the pressures of colleagues, either the pressures that push you to yield to their ways or those that encourage you to opt for approaches that need defense. It sounds trite, but the best way is to be yourself intellectually, wherever that may lead you.

On Being a "Pre-post Modernist"

AYDINLI: Turning to more specific elements of your own personal journey in the formation of methodological identity—an aspect of our overall professional development that I think is extremely important—I would like to ask about your self definition as a pre-post modernist in your most recent book. First, correct me if I am wrong, but in trying to understand your conceptualization of pre-post modernism, it seems to involve:

(a) relaxing the structures of parsimony (a step away from a "scientific" or what we could call a positivist approach),
(b) a commitment to moving from subjective to intersubjective understandings that can allow for consensus (a step away from postmodernists),
(c) the idea that all research is value-laden so we must be explicit about the values that guide what we are doing.

I should admit from the start that I am somewhat uneasy with the title of the chapter that says it is the "confessions of a pre-post modernist." To me this gives the impression that you have changed dramatically, crossing (somewhat ashamedly) from a "positivist" position to a "nearly" postmodernist one (with the implication that you may someday move on to the "full" postmodernist position). This understanding is further encouraged when you say that you went through a "major transformation"—the presumption being that said transformation was from a positivist/scientific position to an almost postmodernist one. But as I was reading the chapter—and as I read more about your calls for bridge-building—I began to wonder whether it might not be misleading to apply a label to yourself that implies you are at least moving toward a particular (postmodernist) position rather than finding a term that either (a) more directly reflects how you have developed the old position or (b) reflects something truly new (bridging).

The bridging you speak of appears to be built on two primary concepts: explicitness (which is undervalued by many positivists) and intersubjectivity (which is undervalued or dismissed by many postmodernists/positivists). Given that this new position seems to truly identify the key strengths and weaknesses of both primary approaches (and thus proposes a bridging), do you feel the terminology you use to define pre-post modernism adequately expresses this bridging ideal?

Overall you provide enough critical comments about postmodernism (for example, your description of postmodernists becoming "rigid and ideological" in their rejection of behavioral approaches) and you do not reject positivistic, "scientific" ideals (for example, when you seem to argue that one can remain scientific while still recognizing that there is no such thing as purely objective inquiry), leading the reader to question your "pre-post modernism" title. Is it really reflective of who you are? And, do you think that postmodernists would accept this title for you? Do you think they might consider you a wolf in sheep's clothing?

Rosenau: Your insights are well taken, except that your comments tend to be overreactions to the label I used. In my mind I used the "pre-post modern" label as a spoof, as a way of suggesting that I fall shy of being a postmodernist without dismissing their insights entirely. More than that, I suppose I wanted to resort to spoof in order to imply that some observers were too quick to pick up on new labels. The speed with which many analysts dismissed "positivism" bothered me because it seemed so anti-empirical. In retrospect, my spoofing only compounded the problem and thus it is NOT an approach I would use again. If my label is misleading, I do regret it, as I certainly do not see myself as moving toward postmodern perspectives.

But you are right in highlighting my commitment to explicitness and intersubjectivity. For me, focusing on these is what the game of research is all about. There is no other if one wants to comprehend the human condition. Knowledge in the social sciences cumulates as investigators make explicit the values, concepts, and methods that underlie their formulations and findings, thus enabling others to evaluate the formulations and findings and serving either as the basis of an emergent intersubjective consensus or as results that are not widely accepted and fail to enlarge understanding. That is the main reason that I included my confessions chapter in *Distant Proximities* (2003). Some might see the chapter as overly egotistic, but I see it as living by the commitment to explicitness. It is true that all too few analysts proceed in this way. One is hard pressed to find a book, or even a paragraph, in

which the author sets forth the personal backgrounds factors underlying his or her work. Come to think of it, I cannot think of any that do this, though it should be a standard procedure to have at least a paragraph in a preface that tells the reader where the author is coming from.

Put differently, I am convinced that intersubjectivity is the only way knowledge accumulates because we do not want subjective interpretations and we cannot obtain objective ones. So we have to settle for those findings, insights, and formulations that thoughtful observers buy into, thus giving rise to a consensus about the ideas or findings involved. This is the reason why we do research and publish: to contribute to the ongoing research on a subject, or to revise it with our findings, or to demonstrate it is erroneous. Knowledge-building is in this sense fundamentally a social process. (As J. Robert Oppenheimer once said, "gossip is the life blood of physics!") Of course, a field can have competing intersubjective consensuses, and in fact most fields do. Such is the nature of knowledge, an ever-evolving series of competing consensuses. To be sure, the adherents of the various paradigms aspire to enlarging their ranks by winning over adherents of the competing paradigms. In this sense the competition among the various consensuses can, at best, be a constructive method of knowledge-building even as it can also, at worst, get vicious and counterproductive.

AYDINLI: I could not agree more. Indeed, I would wager that if such an understanding were the most widespread or common one in our discipline, newcomers would feel a great deal more secure in the face of the many difficult choices I cited earlier. Your definition of intersubjectivity is inclusive enough to allow for the further growth of a sensible mainstream, yet exclusive enough not to let marginal elements confuse the general principles of scholarship and inquiry. I would say that for newcomers it is a tremendously useful image of social science inquiry in general.

Also, in terms of your attempts at explicitness in your confessions chapter, I firmly believe in its value. I think it is worthwhile for us all to take a moment to consider the effect explicitness has on our reading of a particular scholar's works when we first get the opportunity to meet that scholar in person. I was very much influenced during my graduate studies when, for example, Alexander Wendt and Stephen Krasner joined in as guests at our graduate seminars at McGill. After listening to and meeting them, there was no doubt but that our classroom discussions of their works went in different and, I believe, more insightful directions. For better or for worse, it became virtually impossible to read those scholars' works again in the same way as we did when they were just a name, affiliation, and a list of research interests or previous publications. Of course, the latter may provide us with subtle clues about certain aspects of the author's identity, but nothing on the scale of a face-to-face meeting or an explicit attempt by the author at open self-reflection or self-identification.

This experience suggests to me that perhaps we should consider a disciplinary movement to encourage our members to develop and expand the currently accepted genre of the "author's bio note" into something more revealing and explicit than simply affiliation and research interests. I would like to see, for example, some indication of the author's past history, such as where they have worked and lived. Has the author remained all of his or her life in one place? Did he or she take a break along the educational path to join the Peace Corps, live abroad, or work in a different field? I think it would also be valuable to know about some of the author's nonprofessional affiliations or interests. Of course, it would be up to the individual author to determine how many or which of these affiliations to provide, but even that choice would be revealing to the readers and help them interpret the content of the text. In other words, reading that someone belongs to Amnesty International or the Nature Conservancy may not reveal the full "truth" about that person if they also choose to omit that they are a member of a more conservative political party, but it does at least provide us insights into the identity the author likes to put forth

to others. Going beyond the author bio could provide valuable information if the authors were encouraged in their texts to indicate how they came to choose the research topic or particular questions they investigate. Was it simply a personal interest or were there pragmatic issues involved such as a possible future grant? Was the topic of global or current scholarly interest or something sparked by a dinner table conversation?

Returning once more to your confessions chapter, you ultimately conclude that all groups' methodologies are essentially the same—they all "share a readiness to proceed explicitly as they probe whatever problems of world politics they deem significant" (Rosenau 2003: 420). Do you think this belief in everyone's readiness to be explicit could be an overoptimistic one?

Rosenau: You are right, I was overly optimistic, as can be readily inferred from my previous comment. I suppose my bridge-building impulses underlay the optimism, as if one can focus on the areas of convergence and avoid those of divergence. Clearly, however, I am wrong in this regard. As I understand some postmodernists, for example, the background of the author is irrelevant. Indeed, so is the author, given that according to this perspective what counts is the text.

Personal and Disciplinary Transformations

Aydinli: I have always seen you as a scholar of change who happens to specialize in world politics. It strikes me as a pity that we do not even have a subsection in the International Studies Association (ISA) with a focus on change. Do you think such a section would be useful? Would you find such a labeling of yourself an appropriate one, or could you recommend a better one?

Rosenau: Yes, I accept the label. Change is the central condition of our times and, thus, it is a more accurate label than any other that I can think of. My faltering memory tells me that there may have once been a proposal for an ISA section that focused on "transformation," but I do not know what came of it. I do know that the American Political Science Association has a section on Ecological and Transformation Politics and I applaud any effort to create such a section in ISA. It would surely help to call attention to the field's prime dynamics.

Aydinli: On the whole, how accepted a concept do you find transformation to be in IR? How open-minded do you think we are; are we still very much trapped in conceptual jails?

Rosenau: It is my impression that the concepts of change and transformation are neither widely accepted nor widely used in IR (and political science too). I believe the reason is that many analysts do, indeed, remain trapped in conceptual jails that do not include propositions allowing for change. That is a primary weakness of the realist paradigm, but it is also true of other approaches. So we may be open-minded, but our tools curb our ability to express our imaginations and to become committed to theoretical and empirical innovation.

Aydinli: I suppose it is important to note that change does occur, but could we also say perhaps that change is slower than it should be? In a discipline like IR, more so than in most other disciplines, we are issue-bound; we need to try and understand things as they happen. Even potentially radical historic changes can happen rapidly. There is, as a result, a heavy burden on the shoulders of IR scholars to develop our scholarly response to these changes in an equally rapid manner. Of course, I am not suggesting that we turn our theoretical analyses entirely into event-based policy discussions, but it seems undeniable that, particularly in the current era, the traditional time lapse between events and the scholarly study of those events has shrunk. When the world around us is rapidly changing, there are bound to be problems if we maintain the basic instinct to stick resolutely to traditional approaches and theories or even issues stemming from our earlier disciplinary socialization. One need only consider the obvious example of recent years:

before we had wrapped up conclusively our attempts to understand the end of the Cold War, we had to turn to exploring and debating the significance of September 11.

You, of course, have certainly not been one to shy away from change and transformation. Considering the routes you ultimately have taken, you have identified the need to be different as a key element directing your transformations as a scholar. Related to this "need," at least twice in your "confessions" chapter you differentiate yourself from the "norm":

> the assumption that turning points in one's intellectual development are largely responses to climactic moments in world affairs does not apply in my case. (Rosenau 2003:407)

> From postmodern and poststructural perspectives, scientists deceive themselves as well as their audiences when they cast their results in the language of established facts. There is truth in this line of reasoning—except in my case it does not hold. (Rosenau 2003:418)

I wonder two things. First, do you think this need to be different is unique? I recall a colleague once telling me how she asked a class of Turkish graduate students studying cultural issues whether they considered themselves "typical" Turks; not a single one of them said yes. So, if it is true that there is possibly a common desire to be different (or, at the least, not to be "just another mainstream IR scholar"), how does this relate to the contradictory idea that the vast majority clings to "established ideas"? Second, going back to the issue of a need to be different, do you see any risks in such a self-admitted need when it comes to one's academic inquiry?

ROSENAU: My guess is that the need to be different is confined to a few analysts. And sometimes I think it is a dangerous tendency. To be different for the sake of being different is as rigid as the inclination not to be innovative. On occasion, at those times when I feel insecure about a new formulation, I worry this may be true of me and I try to contest the need to be different by endlessly asking myself whether I am saying or writing this sentence or paragraph just to be different, merely to shock the conventional among us. There is pleasure to be had in challenging and shocking people in the mainstream, but it is essentially a transitory and counterproductive pleasure. It takes one's eyes off the main goal of expanding our understanding of how the world works.

The risks for me personally are not very great, partly because few will probably read my "confessions" chapter and partly because I am too near the end of my career and too set in my ways for such risks to be relevant. For younger scholars, however, the risks may well be considerable, especially if the need to be different leads them to absurd intellectual perspectives (indeed, this may be the case for some postmodernists).

AYDINLI: I am curious as to which intellectual perspectives you are referring to when you say "absurd," especially with respect to postmodernists.

ROSENAU: I have in mind those who ignore the virtues of being explicit and value merely giving voice to their subjective perspectives without labeling them as such.

AYDINLI: Do you think this need to be different is the major source for your being always "ahead of others"—something I believe John Ruggie once said about you?

ROSENAU: Wow, now there is a tough question! John is very kind, but I am not sure whether I have moved ahead or fallen behind others. If it is the former, I am reluctant to ascribe it to a psychological need rather than a set of intellectual insights—or at least I am reluctant to admit as much. But let me say, doubtless psychological dynamics are at work in all of us. I have just been a little more willing to acknowledge them, or perhaps a little more willing to speak about what underlies my intellectual commitments.

Aydinli: And do you really accept the assumption that IR scholars' intellectual turning points correspond to turning points in world affairs?

Rosenau: I know I made such a statement in my intellectual autobiography, that is Chapter 19 in *Distant Proximities* (2003), but on second thought that may be inaccurate. Our conceptual jails tend to be too well constructed to allow turning points in world affairs to upend them. Turning points in world affairs do give pause and some (like September 11) may lead to revised thinking about the dynamics that sustain the course of events. But such revisions usually lead to reaffirmations of the existing paradigms with new wrinkles or qualifications. I am really not sure of all the factors that led me to turn in new directions. To revert to the earlier metaphor, I guess the fire in my belly became a wildfire that expanded across new terrain. In addition, as I have already indicated, the anomaly file was surely part of it and a sense that existing paradigms were insufficient was also an important motivator.

Aydinli: Still on the subject of change, you describe your 1997 book *Along the Domestic-Foreign Frontier* as stressing the idea that a frontier has been created where the domestic and foreign converge. This concept seems a follow up on your earlier linkage politics arguments, so to what extent does that particular move on your part represent continuity or change in your research inquiries?

Rosenau: I have always felt that the unifying theme of all my work is the overlaps, links, connections—whatever they may be called—between domestic and foreign affairs. My 1969 edited book on *Linkage Politics* may seem to be the origin of this line of thinking, but actually it is first manifest in the 1963 paper I wrote entitled "Pre-Theories and Theories of Foreign Policy" that was published in 1966 in a book edited by R. Barry Farrell. That may be the best essay I have ever written. It still serves to organize my thinking about foreign policy. And I believe it may do the same for more than a few others.

Aydinli: On the same general issue, why do you think that IR as a discipline remains so insistent on keeping the distinction between domestic and foreign?

Rosenau: Because the state is still regarded as the prime and dominant entity in world affairs and, thus, the discipline is organized along the lines of that distinguishing feature. Moreover, these are the same lines around which governments are organized. All states have agencies or departments devoted to domestic problems and others devoted to foreign problems, thereby giving rise to interagency committees rather than wholesale governmental reorganization that allows for the overlap of the two central domains of national and societal life. The new US Department of Homeland Security may be a portent of change in the direction of agencies that embrace both domestic and foreign challenges.

Aydinli: Your writings, by being rooted in transformation and in the "proposition that these transformations continue to unfold," give the impression of work with an unending, unreachable destination. We are left to trace the paths of these transformations, but by the time we catch a glimpse of some result, the "current reality" has already changed. It is like looking at a star and knowing that the light we are seeing left the star so many years ago that in fact the star itself may no longer even exist. So, what do you see as the biggest challenges for one who focuses his or her research agenda on transformation?

Rosenau: Yes, our endeavors involve striving for unreachable goals. But that ought not be off-putting. There is pleasure to be derived from continuing the quest even if one knows it can never be adequately realized. In the nature of things, knowledge is transitory, endlessly evolving, endlessly shifting emphases, as the intersubjective consensuses undergo transformation; for me, that is a major reason why its pursuit is so exciting. The biggest challenge of such a commitment is conceding that the task has no end and realizing that the quick obsolescence of knowledge means our contributions will be fleeting and only briefly meaningful. Few are the scholars whose contributions continue to have consequence once they are no longer probing and their lives come to an end. Karl Deutsch, for example, was a

major figure in IR because his publications were so innovative, but he is rarely cited today and doubtless few in the younger generation have even heard of him, much less read his writings.

AYDINLI: On the same topic, but going back to your decision to label yourself as a "pre-post modernist," I wonder if you see any relation between your inquiries focusing on issues that are constantly in the process of taking shape and a possible frustration with observation methods that seem incapable—due perhaps to conceptual time lags or even just practical problems such as dissemination procedures—of keeping up with the pace of change. Could such a potential clash have led you to feel that you can never really catch up with the transformations you seek to understand, and thus postmodernism expresses better the frustrated feeling that may result? Basically, could the elusiveness of the subject have led you in any way to a certain methodological positioning?

ROSENAU: I have already said that the pre-post modernist label was essentially a spoof. But the elusiveness of the subject does lead me often to ponder the question of how we can develop a methodology capable of capturing the complex, nonlinear nature of IR today. I think the answer lies in agent-based modeling through computer simulations, but I fear I am too old to tool up in this regard. The most I can do—and I do try—is to impress upon students the need to tool up in the computer sciences. Most are reluctant and reject the advice (indeed, many come into IR in order to get away from mathematics and computational science), but a few have bought into the idea in recent years with compelling results.

For years now, in every seminar I teach I have students read M. Mitchell Waldrop's *Complexity* (1993) as their first assignment, and I tell them that reading the book will change them. The book tells a remarkable and provocative story and discussions of it are usually extremely exciting and eye-opening. At one level it tells the story of the Santa Fe Institute and its founders, distinguished leaders in various disciplines who managed to develop a new theory that offers insights into the meaning of life and the underpinnings of our complex world today. In so doing, at another, more abstract level, it raises profound and provocative philosophical issues that bear on world politics as well as many other dimensions of the human condition. Little wonder, then, that each semester a few of the students become so enamored of complexity theory that in fact they do change their orientations and, in some instances, their curriculum!

AYDINLI: The question "of what is this an instance?" which you often raise in your presentations at seminars and conferences seems to be the only constant for you; why do you think that is? Is there a chance that your methodological identity lies in this question and, if so, how would you describe the identity that it suggests?

ROSENAU: Yes, that question is a constant for me. It not only dominates my intellectual inquiries, but it also pervades my personal life—riding elevators, walking streets, eating in restaurants, observing strangers, to mention only some of the settings in which I ask about what I observe "of what is this an instance?" So, yes, it is my methodological identity. Its prime virtue is that in asking the question about whatever one observes one is driven to climb up the ladder of abstraction in order to find a larger category into which to classify what one has just seen. This exercise is the route to theory, a route that I enjoy traversing. I view the higher rungs of the ladder of abstraction as the locations where the interesting questions and phenomena lie, whether they are in our personal or professional life. It is on the higher rungs that the mind comes alive, which is not to denigrate the lower rungs where empirical materials are found. Perhaps some people have their minds come alive at the lower rungs, but I find the rarified air at the top of the ladder exhilarating.

AYDINLI: I know I speak not only for myself but for others who have had the opportunity to work with you, when I say that your question "of what is this an instance?" is a great contribution to teaching and research. It gives a clear starting point in the mess of world politics. It offers us something to cling to. I think that

regardless of one's stage of disciplinary socialization, when you're perplexed, asking a good methodological question like this one allows us or guides us to ask good research questions.

Another phrase, or in this case a word, that I often associate with you is "checkableupable." In your confessions chapter you relate being "checkableupable" to being explicit, which you say is "at the heart of science" (Rosenau 2003:413). Throughout the piece you talk about relaxing your "scientificness" but you remain loyal to "checkableupableness." The latter term seems scientific to me, what does it mean to you?

Rosenau: There is no contradiction whatever between the two concepts. I argue that we need to relax our criteria of parsimony, not that we should give up on being scientific. The *Turbulence* (1990) book contends that the mounting complexity of world affairs requires us to loosen our criteria rather than cling to just a few (as realism does). Thus, for example, I write about eight major sources of globalization in the *Distant Proximities* (2003) book, but I am not prepared to suggest there are hundreds or even a dozen—that would be carrying relaxation too far. An encyclopedic enumeration of variables is as counterproductive as an excessively parsimonious one. "Checkableupableness" is my way of calling attention to the need for explicitness. That is what the term means to me.

Aydinli: What is so magical about eight major sources? Why would a dozen be too many? How does one know when to stop enumerating relevant variables?

Rosenau: It may sound evasive, but one stops identifying the crucial variables when the mind comes to rest, which is another way of saying that one stops when one is persuaded that the identified variables account for most of the variance one's theory seeks to explain. Given the complexity of world politics, it is highly doubtful that one's comprehension of IR is so creative and thorough that one can account for one hundred percent of the variance. So there is nothing magical or mystical about the number of variables one comes to regard as crucial. To repeat, one stops when one is confident that the interaction among the specified major variables accounts for most of what drives the course of events.

In attempting to probe the dynamics of globalization, I stopped at eight variables because each of them is operative in every country of the world. There are crucial issues high on the global agenda—such as nuclear proliferation—that surely need to be investigated, but they are not relevant to policymaking in every country. But the eight variables do meet the every-country criterion. These are fully elaborated in Chapter 3 of my *Distant Proximities* (2003). They consist of what I call the skill revolution; the organizational explosion; authority crises; the bifurcation of global structures; technologies that have shrunk time and distance; the mobility upheaval; the weakening of states, territory, and sovereignty; and the shift to neoclassical economic policies. Some of these are central to the turbulence formulation that I developed in the late 1980s, but it was not until I subsequently became preoccupied with the dynamics of globalization in the mid-1990s that I began to bring the eight together as the basis for an overarching theoretical framework.

Aydinli: Speaking of the *Turbulence in World Politics* (1990) book, you once said that you consider it your most important book. Where were you in your methodological positioning when you were writing this book? Would the result have been different if you were writing it now? Would it still be your best book?

Rosenau: By the time I started writing that book I had moved on from a commitment to a very strict scientific approach. Indeed, as I have already said, in the second chapter of that book I argued for the need to relax our criterion of parsimony. As for it still being my best book, I think it is for several reasons, not the least being that *Turbulence* (1990) turned out to be the first in a trilogy, though the second, *Along the Domestic-Foreign Frontier* (1997), and the third, *Distant Proximities* (2003), were not planned to be sequels. Only in retrospect do they loom as a trilogy, with the continuity being that, unbeknown to me at the time, they successively

narrow in on micro actors and probe the ways in which individuals at the micro level shape, and are shaped by, collectivities at the macro level. So *Turbulence* (1990) seems important partly because it turns out to be innovative and creative through the framing of a variety of new and (I think) important concepts (such as a bifurcated world, the skill revolution, and the relevance of pervasive authority crises), but also because it paved the way for the two books that followed. There are a couple of lesser points I would change in *Turbulence* (1990) if the publishers would agree to a revised edition (which they will not because it still sells fourteen years later). On balance, though, I would not alter the prime thrusts of the book. Indeed, developments since 1990 have more than affirmed the core of the turbulence model. In the last fifteen years, for example, individuals as migrants, terrorists, and mobilized activists have become increasingly conspicuous on the world stage. Likewise, the global stage has become increasingly crowded with a huge number and variety of nongovernmental organizations that have pressed governments into adopting new perspectives and policies, thus highlighting the ever-greater relevance of bifurcated global structures, much as the turbulence model anticipated. So yes, I still regard it as my best book, though I am also very proud of those that followed.

AYDINLI: Perhaps one final methodological question, you refer in your "confessions" chapter in *Distant Proximities* (2003) to a paper on the fiftieth anniversary of the United Nations (Rosenau 1996) in which your of-what-is-this-an-instance question sounds like it emphasizes an historical approach. Yet this is something you do not often do in your work it seems. Have you ever been "accused" of an apparent ahistorical perspective? What do you think about such an argument?

ROSENAU: Yes, I have often been accused of being ahistorical, especially by several of the essays in the book Heidi Hobbs (2000) edited entitled *Pondering Postinternationalism*. And there is some truth to the charge. Its reality derives from two sources: (a) my reading of history is limited and (b) I think analysts are much too quick to explain events and trends as part of historical patterns. The second reason may be a function of the first, I suppose, but I have long developed an elaborate conviction that historical analogies (and, after all, that is all that historical explanations of current patterns are) can be very misleading, that the variables at work in previous eras may not be at work in the present or they may operate differently. It has always seemed to me, for example, that the US intervention in Vietnam was founded on the premise that because such an intervention worked in Korea, a similar outcome could be expected in Vietnam, an analogy that clearly proved to be inaccurate. Stated more generally, in order to apply the lessons of history, one needs a theory of when a lesson is applicable and when it is not. But as far as I know, such a theory of history's lessons does not exist, thus inhibiting any attempt to meaningfully ransack history for relevant lessons. Of course, if by chance one develops a theory of history's lessons, its premises should be made explicit, a task that staggers the imagination.

AYDINLI: This conversation has been genuinely fascinating for me, hearing the details about and gaining insights regarding the various stages and turning points in your academic career to date. Even more interesting to me is the impression I have that the changes you have made have been part of a smooth—rather than tumultuous or terribly painful—process of transformation. Your ability to do this perhaps seems smooth because of what I see as one of your remarkable traits, namely, the ability to avoid becoming entrenched in the jails of a single paradigmatic or methodological identity (a trap that proves comforting to so many scholars), but rather to remain open to change and innovation. Your scholarly identity seems directed at an ideal of "good" scholarship that remains unbounded by particular questions, ways of asking them, or ways of answering them. Your example can certainly provide encouragement to newcomers in the discipline who may be feeling pressured into unwanted identities or lost because they do not seem to fit

into a singular identity. It seems appropriate to end by asking what we might expect next from you on your never-ending transformative journey?

Rosenau: That question has been very much on my mind of late. The answer has two dimensions, one practical and the other intensely personal. The practical dimension includes three unfinished projects. One is a quantitative study of how US leaders contribute to and are shaped by the dynamics of globalization that several colleagues and I have undertaken. This topic has yet to be systematically explored. We have written and expect to publish a book entitled *On the Cutting Edge of Globalization: An Inquiry into American Elites* (Rosenau et al. forthcoming). After finishing *Distant Proximities* (2003), I started writing a book entitled *People Count*. I wrote some seven chapters, but then got diverted to other projects and have yet to be moved to return to it. I am also attempting to compile a two-volume collection of my essays that have not been published or have appeared in more obscure journals.

This last project involves looking back and highlights some very personal considerations. At some point one has to face the question of whether what seems like a never-ending journey is, in fact, coming to an end and, if so, when to acknowledge that it is over. I have often said I wanted to follow the example of Ted Williams, perhaps one of the greatest baseball players of the twentieth century. In March of 1960, after a career of nineteen years, Williams announced that the coming season would be his last. Most ball players do not stop until their talent declines to the point where their services are no longer sought. But Williams announced that the 1960 season was his last and, then, on the last day of that season at his last at bat hit a home run! That is a model for me because I do not want to keep writing and researching if the quality of my work declines. I want my last paper or book to be the social science equivalent of a home run. Thus, I need to pause and ponder how to proceed from here into the future. I am dominated by the realization that not much time remains, even as it would be good to use what is left in the most constructive way I can, and that requires some heavy thinking.

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